

The Corporation and Industrial Evolution

Microanalytic Business History

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My remarks are brief and rather informal -- neither of which is necessarily a bad thing. In any event, I hope that the paper is responsive to the spirit with which this session was conceived.

I begin with an autobiographical sketch of how my contact with business history developed and of the impact this had on my own research. The research ramifications of joining transaction-cost economics with business history are then set out. Concluding remarks follow.

BACKGROUND

The business history literature, especially the work of Alfred D. Chandler, Jr., has had a major impact on my research. Just how great can best be appreciated by tracing briefly the origins and the evolution of my interests in the business firm.

My interest in studying the modern corporation initially took shape during my graduate education at Stanford (where I received an MBA) and even more at Carnegie-Mellon (where I received my Ph.D.). My dissertation, "The Economics of Discretionary Behavior: Managerial Objectives in a Theory of the Firm" (1964), was the first published evidence of these interests. The dissertation took seriously the Berle and Means inquiry: "have we any justification for assuming that those in control of a modern corporation will also choose to operate in the interests of the stockholders" [4, p. 121]. Rather than assume that firms are operated in a relentlessly profit-maximizing fashion, I postulated instead that the managers of firms were often in effective control and that they maximized a managerial utility function (of which profits was one argument).

The exercise yielded some interesting insights and some of the data seemed to bear these out. I applied the model to defense contracting (1967) [23] and developed a dynamic-stochastic version (1968) [24] in subsequent years. I was also intrigued by the issue of how the exercise of managerial discretion might be checked. Competition in the capital market was one possibility, but this was subject to numerous limitations. Competition in the product market was another, but this was arguably weak in dominant-firm and highly oligopolistic industries.

A possibility that I did not consider until I came upon Chandler's remarkable book, Strategy and Structure [9] was that the corporate control dilemma posed by Berle and Means might be significantly alleviated by internal organizational form changes. As you are all aware, Strategy and Structure records the transformation of the American corporation from its traditional (or unitary) form to its modern (or multidivisional) form. Although many other students of the corporation were aware that this transformation was in progress, none had the depth of knowledge which Chandler possessed. And, Chandler aside, only Richard Heflebower and Armen Alchian [1] had glimpsed the economic significance of the M-form structure.

It was untenable, however, after the publication of Chandler's book, to hold that the internal organization of the corporation was economically unimportant. In the language of transaction-cost economics, the large, unitary form structure was beset with both bounded-rationality limits and opportunistic distortions. The ability of the management to handle the volume and complexity of the demands placed on it became strained and even collapsed in the large U-form enterprise. This was a manifestation of bounded rationality. Furthermore, managers of the functional parts were sometimes given to subgoal pursuit at the expense of overall enterprise achievement, which reflects opportunism. The creation and refinement of the M-form concept served to correct both. The core argument is this [26, pp. 1555-56]:

The M-form structure fashioned by du Pont and Sloan involved the creation of semi-autonomous operating divisions (mainly profit centers) organized along product, brand, or geographic lines. The operating affairs of each were managed separately. More than a change in decomposition rules was needed, however, for the M-form to be fully effective. Du Pont and Sloan also created a general office "consisting of a number of powerful general executives and large

advisory and financial staffs" [9, p. 460] to monitor divisional performance, allocate resources among divisions, and engage in strategic planning. The reasons for the success of the M-form innovation are summarized by Chandler as follows:

The basic reason for its success was simply that it clearly removed the executives responsible for the destiny of the entire enterprise from the more routine operational activities, and so gave them the time, information, and even psychological commitment for long-term planning and appraisal... [The] new structure left the broad strategic decisions as to the allocation of existing resources and the acquisition of new ones in the hands of a top team of generalists. Relieved of operating duties and tactical decisions, a general executive was less likely to reflect the position of just one part of the whole [8, pp. 382-83].

In contrast with the holding company -- which is also a divisionalized form but has little general office capacity and hence is little more than a corporate shell -- the M-form organization adds (1) a strategic planning and resource allocation capability and (2) monitoring and control apparatus. As a consequence, cash flows are reallocated among divisions to favor high yield uses, and internal incentive and control instruments are exercised in a discriminating way. In short, the M-form corporation takes on many of the properties of (and is usefully regarded as) a miniature capital market, which is a much more ambitious concept of the corporation than the term "holding company" contemplates.

Regarding the corporation as a miniature capital market puts a different light on the corporate control dilemma. Might it be the case that managerial discretion had been checked more effectively by internal than it had been by regulatory or external organizational reforms? That possibility plainly had to be faced; but I, among others, was slow to embrace it -- since it had massive ramifications for the research agenda. I was nevertheless broadly persuaded of the merits and began to de-emphasize (U-form) managerial discretion arguments as my

understanding of the consequences of the M-form structure for the organization and performance of the corporation deepened.

A few years later, and largely independent of my concerns with M-form organization, I began to restudy the rationale for vertical integration. This arose in conjunction with a graduate course on "Theories of Institutions" that I was teaching in which the organization-theory and market-failure literatures were central. A comparative institutional orientation was adopted. Ronald Coase [7] excepted, prior work on vertical integration had mainly emphasized technology. Joe Bain's treatment was typical. Thus he argued that vertical integration was warranted where production had a "physical or technical aspect," as in iron and steel making -- where thermal economies were said to exist. Where, however, such technological features were missing, "the case for cost savings from integration is much less clear" [3, p. 381].

Inasmuch, however, as blast furnaces and rolling mills are technologically separable stages, common ownership does not follow as a matter of technological necessity. In principle, each stage could be owned separately and product exchange could be mediated by contract. A decision to supplant autonomous contracting by vertical integration is thus explained not in terms of technology but by the differential ease of reaching agreement and, even more, of adapting to unforeseen (and often unforeseeable) disturbances between site-specific stages of production. Attention is thus shifted from technology to an assessment of alternative means of "contracting" -- or, more generally, of locating transactions within an appropriate governance structure. The interesting problems of economic organization are thus comparative institutional ones and turn mainly on transaction-cost economizing.

Assessing the limits of alternative means of contracting involved a more microanalytic approach to economics than was characteristic of neoclassical economics. Partly this was a matter of reshaping the behavioral assumptions to bring them into closer correspondence with what Frank Knight has referred to as "human nature as we know it" [15, p. 270]. And partly this entailed self-conscious examination of the underlying attributes of transactions. Both aspects are discussed elsewhere [25]. Suffice it to observe here that the strategy of studying economic organization in transaction-cost terms essentially reduces to an effort to assign transactions to governance structures in a discriminating (mainly a transaction-cost-economizing) way.

That this approach could be used to interpret organizational innovations reported in the business history literature was

first suggested to me by a student, Harriet Moses. She called my attention to John Buttrick's fascinating treatment of inside contracting in the Journal of Economic History (1952) [6]. Buttrick described both the nature of the contractual relations and the factors that gave rise to the demise of inside contracting in sufficient detail to permit transaction-cost reasoning to be brought to bear. I was convinced at this juncture that business history and transaction-cost economics were well suited to the needs of each other.

Efforts to turn up further examples were not, however, altogether successful. My economic history colleagues were mainly interested in other things. Help from an unexpected source, however, appeared. The radical economics literature dealing with the organization of work -- Stephen Marglin [19], Katherine Stone [21], and Samuel Bowles and Herbert Gintis [5] -- turned out to be instructive. Their argument was that if technology was not determinative of hierarchical organization, class conflict presumably was. My view was that economizing involved more than choice of technology and that transaction costs also needed to be considered. Thus, whereas Marglin asserts, among other things, that "an end to embezzlement and like deceits changed the division of the pie" but lacked efficiency consequences [19, p. 51], I contend that systems that are subject to embezzlement and like deceits invite organizational reform for efficiency reasons. As between two modes of work organization, one of which provides more effective checks against opportunism than the other, total product will be greater under the work mode where promises and deeds are in closer correspondence, ceteris paribus. Such an approach to the study of work organization disclosed that comparative institutional assessments of evolving organization forms from a transaction-cost point of view had wide application.

Chandler, in the meantime, had another major book on American business history in progress. The Visible Hand [10] dealt with the period 1850 to 1920 and successively traced the progressive evolution of hierarchical structures through the railroads and manufacturing to include selective forward integration into merchandising by some industries. I not only had the benefit of seeing this book in page proofs but participated in a pre-publication conference dealing with the visible hand theme [11].

The Visible Hand provided considerable grist for the transaction-cost economics mill. It was, both for this reason and for its intrinsic merits, a delight to read. I confess, however, that I was not fully persuaded by the explanations that Chandler provided for some of the organizational developments

that he so carefully recorded. To be sure, the transaction-cost economics apparatus upon which I had been working was still primitive. It was, nevertheless, "available" and could have been brought to bear systematically. Put differently, I had a sense that traffic between business history and transaction-cost economics was a two-way street. And I was hopeful that it could persuade Chandler, among others, to see things this way.

RESEARCH RAMIFICATIONS

As the preceding remarks suggest, business history and transaction-cost economics are, in important respects, made for each other. The reasons are not hard to find. Both deal at a similar microanalytic level of analysis. And while business history stands in needs of "an established system of theory" [18, p. 135], transaction-cost economics has need for relevant institutional data.

Mainline price theory plainly did not meet the theoretical needs of business history. And most works in economic history operate at too high a level of aggregation to be evaluated in transaction-cost terms. My efforts, for example, to discern the characteristics of early business forms from a reading of The Unbound Prometheus [17] or The Rise of Capitalism [16] were largely unsuccessful. And even the important work of Lance Davis and Douglass North [12], which is expressly concerned with supplying an economic rationale for observed institutional change, is too aggregative for what I perceive the needs of transaction-cost economics to be.

Transaction-cost economics holds that the transaction -- which is a much more microanalytic unit than a good or service -- is the appropriate unit of analysis [8]. Transactions, moreover, are to be evaluated in a comparative institutional context, which requires that alternative governance structures be described in sufficient detail to permit the contracting ramifications of each to be assessed.

To be sure, stating the economic problem of organization as mainly one of transaction-cost economizing oversimplifies. Certain objective features of the technology are important and need to be taken into account. Also, economic activity is undertaken with the prospect of realizing a revenue stream, which is to say that the benefits as well as the costs matter. And power as well as efficiency may be driving organizational reforms. I nevertheless maintain that the importance of these other features is easily overstated and that, for the present at least, maintaining a transaction-cost economizing focus is warranted.

Thus, consider technology. It is my contention that large size is rarely warranted by reason of technological features alone. This is because (1) technological nonseparabilities rarely extend beyond a group of relatively small size (of which manual freight loading is the usual example) and (2) the decision to join technologically separable units under common ownership necessarily reflects transaction-cost economizing or strategic considerations. Since strategic purposes can be served only if rather special structural preconditions are satisfied -- mainly high concentration coupled with high barriers to entry -- the transaction-cost explanation is presumably the more widespread.

The matter of power is of special interest to radical economists and organizational-behavior specialists. There are two problems with this approach. First, and most important, it has little predictive content. Second, it frequently focuses on initial impact and does not trace the consequences of organizational innovations (including imitation by rivals) to completion. Thus, although efficiency gains will generally be reflected in enhanced profits initially, this is scarcely a durable-power outcome if rates of return are subsequently reduced to competitive levels as a result of imitation by rivals. Conceivably, power theory will be improved in predictive respects, and users will become more careful in assessing long-run effects. As matters stand presently, however, power theory informs the study of organizational innovation very little.

I return, therefore, to the proposition that organizational innovation should mainly be viewed through the lens of transaction-cost economizing. If this viewpoint is accepted, studies of organizational change will need to attend to the underlying attributes of transactions. The three dimensions that I have found useful for describing transactions are (1) the frequency with which they recur, (2) the uncertainty to which they are subject, and (3) the degree to which they are supported by investments in transaction-specific assets.

A considerable amount of the explanatory power turns on this last. The issues here get somewhat technical. Suffice it to observe that (1) asset specificity can and does take a variety of forms and that the organizational ramifications vary with each, and (2) attention to asset specificity is absolutely critical if headway is to be made in applying transaction-cost arguments to business history events. As Armen Alchian has recently put it, "the whole rationale for the employer-employee status, and even for the existence of firms, rests on [asset specificity]...without it there is no known reason for firms to

exist" [2, pp. 6-7] -- and he goes on generously to observe that "Markets and Hierarchies [gives] by far the most elegant, though abstract, statement of the principle" [2, p. 7].

While I am scarcely qualified to describe the research agenda for business history, a strategy that would appear to me to have merit would involve (1) taking observations at a sufficiently microanalytic level of analysis that transaction-cost features are exposed, (2) maintaining an economizing orientation (not, however, to the neglect of other plausible hypotheses), and (3) reporting on organizational innovations that are failures as well as those that are successes.

This last is of special importance. The records mainly register the successes, however. This is unfortunate, since we can often learn as much or more from the failures. One strategy for getting at these failures is to attempt to develop a logical ordering of organizational structures from the most primitive to the most advanced. It may be possible (indeed, I submit that I have actually done this) to observe gaps in the sequence for which a form is not recorded but logically ought to appear. This then becomes a candidate for historical sleuthing.

Among the particulars that warrant attention are: (1) life-cycle features, making allowance for the possibility that specific asset investments that are needed at one time may be greatly reduced as an industry develops and standardization becomes more widespread; (2) the progressive refinement and reshaping of hierarchy, in response, for example, to changes in communication and computation technologies; (3) the limits to firm size (as experienced, for example, by the Ford Motor Company under Henry Ford); (4) studying the organization of labor, with attention both to the appearance of unions and to differences in the degree of formal structure that are associated with unions; and (5) the evolution of "nonstandard" contracting forms, of which cost plus contracting and the use of product-exchange agreements are examples.

As matters stand presently, the market failure literature is in much better shape than is the bureaucratic failure literature. The help of sociologists and organization theory specialists is needed to develop the latter. But business history specialists, especially those informed by a transaction-cost perspective, could themselves add considerably to our knowledge and understanding of the origins and consequences of bureaucratic failure by making such failures the object of investigation. The study of labor organization, with respect to both failures and successes, is also an area that is ripe for investigation from a transaction-cost economics point of view -- as Joseph Reid, among others has recognized.

CONCLUDING REMARKS

Only time will tell whether business history of a transaction-cost kind will flourish. I naturally hope that it will. Both direct and indirect benefits can be anticipated. The indirect benefit is that, inasmuch as transaction-cost economics is still in a primitive stage of development, refinements in the theory can be expected as additional phenomena are brought under scrutiny. The direct benefit is that the study of business history will experience new vitality.

A deeper understanding of the evolution of capitalist forms of organization will naturally result from such an undertaking. Western economies will commonly be studied in this connection. But organizational practices, past and present, in non-western economies can also be studied to advantage. Not only can organizational changes in advanced socialist economies (Yugoslavia, for example [13]) be usefully studied in transaction-cost terms, but underdeveloped economies (for example, Morocco [12] or Vietnam [19]) can likewise be included. To be sure, this stretches the business history domain; but economists are notorious for their imperialism.

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